geologists who supposed that Major Powell had abandoned his extraordinary position on the question of coloration of geological maps. We seem to see in Florida a good representation of the Archean, Paleozoic and Mesozoic beds, as well as the Cenozoic. For this Mr. Dall is in no way responsible. It is a pity that expense should be incurred in printing such maps, since they will have to be republished with the customary colors.

Cary on the Evolution of Foot Structure.[5]—We have in this paper a study of the fore foot of Palæosyops, from a specimen in the museum of Princeton College, conducted with a view of ascertaining the mechanical relations of the parts when in action. The ultimate object is to determine whether the structures presented (facets, etc), can have been produced by direct mechanical impacts, strains, etc., as is alleged by the Neolamarckian school of evolutionists. The study is conducted with care, so far as it goes, but it is not always easy to understand the drift of the author's argument. He reaches but one definite conclusion, viz.; that the trapezoid is too small to express properly a result of direct mechanical causes. This fact, the author says is incompatible with the Lamarckian principle. He informs us that in reaching this result he has applied geometrical methods. "First, the volume of the bones was got at. Next the area of the bearing surfaces and their inclination to the digits were measured. Then giving to the thrust of each metacarpal a value proportional to its volume, the distribution of that thrust can by resolution and composition of forces, be traced through the foot, and the pressure on each surface and bone approximately obtained." Further than this the author does not explain how he reached the result that the trapezoid is too small. It is quite essential that this demonstration should be given if we are expected to accept his conclusion. An essential part of the problem is, however, unnoticed by Mr. Cary; and that is the condition of the trapezoid in the reptilian ancestors of the Mammalia. The phylogeny of an element must be known, since it furnishes the "physical basis" of the problem.

Mr. Cary then proceeds to criticize the explanations offered by Professor Osborn and myself, in accounting for the origin of certain structures. He finds our explanations to be self-contradictory, and that we also contradict each other. Osborn has supposed that the conules of the molars are produced by friction of the molars of opposite

[5]A study in Foot Structure; by Austin Cary. American Journal of Morphology Dec. 1892, p. 305.

series on each other. I have expressed the opinion that the shear of the sectorial teeth of Carnivora was produced by lateral friction during vertical movement of the lower tooth on the upper. I have also asserted that the forms of facets of limb articulations are due to pressure. Mr. Cary sees here the attempt to explain the origin of totally different structures through identical mechanical processes, and believes that the attempt is a failure. Were the conditions of the problems alike, as Mr. Cary thinks them to be, he would have good reason for this opinion. But the conditions in the three cases are entirely different, and our author's conclusion is due to neglect of the elementary facts of the proposition.

The development of conules at the points indicated by Professor Osborn, has been supposed by him to be due to friction between existing ridges of enamel which cross each other when in action, at the points in question. In the case of the development of the sectorial shear, the faces between which the shearing motion takes place are smooth, and without ridges or crests. Hence the entire surface receives a homogeneous friction. In the third case, that of the foot articulations, there is no friction, but there is pressure which when abruptly applied in movement becomes impact. There is really no parity between the three cases.

The author of this paper also thinks that the explanation of the elongation of bones through use of different kinds is not a permissible hypothesis. He cites my attempt to account for the elongation of the leg bones of higher mammals through impact-stimulus; and of other limb bones of other mammals through stretching. But he does not prove that similar results may not flow from mechanical stresses applied in different ways. I suppose that any mechanical stress which determines nutritive processes to a part, will increase its size, caeteris paribus; and the stretch as well as the impact has this effect.

Use is a term which is too indefinite for purposes of exact demonstration, and I have endeavored to reduce it to precision so far as regards the skeleton, by defining it as "friction, pressure and strain." Precisely how these processes affect nutrition is not yet clear. We refer the production of various animal fluids to "secretion", knowing that the products of secretion are most various, as bile, gastric juice, saliva, etc. The exact cause of the diversity remains unknown. So with the effect of stimuli on bone nutrition, we see the cause and the effect, but the ultimate process, as in all nutrition, has as yet eluded our view.

In concluding, Mr. Cary admits one of the two contentions of the Neolamarckians in his two closing propositions. He says "Plasticity

of bone, using the word *plasticity* not in a physical sense merely, but to include absorption under pressure, will probably account for much structure in the foot and elsewhere, especially the connection with the joints, and in the fields of variation and correlation." In the second proposition he says that facts have been adduced by him which are inconsistent with the theory that the size of bones has been increased by the stimulus they receive, and with the theory that regions of growth are determined by regions of pressure and strain. "The testimony of the literature as to the latter point he says is conflicting." I have shown that the supposed conflict is due to a misunderstanding on the part of the author of this paper. The proposition that pressure does not affect growth is in contradiction to the admission made by the author in his first proposition, where he admits that pressure determines structure; for in such change of structure there is always growth. Finally Mr. Cary remarks "That race changes follow those produced in the individual life, or that they are directly caused by their mechanical surroundings, I do not think it has been satisfactorily shown." The fact that the characters of bone structure admitted by Mr. Cary to have had a mechanical origin appear in the young before birth, is evidence that race characters are produced, and that they are produced by mechanical surroundings.

Such criticisms as are contemplated by the author of the paper reviewed above, are important and are what the subject needs. It is along the line followed by him that the ultimate demonstration of the problems involved will be made. We trust that we shall hear from him again in this field, and that in his labors he will be well supplied with the phylogenetic details as a foundation.

E. D. COPE.

Earle on the Species of Coryphodontidæ.[6]—In preparing this paper Mr. Earle had the advantage of the use of the material in the collections of the New York American Museum of Natural History, and the private collection of Professor E. D. Cope. He presents us with a brief résumé of the results of his comparisons, and adds considerably to our knowledge of the characters of the skeleton and dentition of some of the species. He gives a list of the described species, which number twenty-one, and which were referred by Cope to five genera. He concludes that these should be reduced to ten

[6]Revision of the species of Coryphodon, Art. xii, Bull. Am. Mus. Nat. History New York, iv, pp. 149–66; Oct. 18, 1892.